

## Looking to the Future: Will Behavior Analysis Survive and Prosper?

Alan Poling

Western Michigan University

Behavior analysis as a discipline currently is doing relatively well. How it will do in the future is unclear and depends on how the field, and the world at large, changes. Five current characteristics of the discipline that appear to reduce the probability that it will survive and prosper are discussed and suggestions for improvement are offered. The areas of concern are (a) the small size and limited power of the discipline, (b) the growing focus of applied behavior analysis on autism spectrum disorders and little else, (c) the esoteric nature of much basic research, (d) the proliferation of “applied” research that really isn’t applied, and (e) the widespread use of imprecise and potentially harmful technical language.

*Key words:* behavior analysis, terminology

Behavior analysts like me claim that their approach to studying, understanding, and improving the actions of humans and other animals is scientific, rational, and powerful (Poling, Schlinger, Starin, & Blakely, 1990; Schlinger & Poling, 1998). If that is indeed so, then the future of our discipline should be bright. Perhaps it is. Membership in the Association for Behavior Analysis International (ABAI) has grown substantially over the years, and the rate of growth has increased recently. There is a heartening acceptance of Board-Certified Behavior Analysts (BCBAs) as legitimate professionals with significant skills and areas of unique expertise, most notably in providing services to people with autism spectrum disorders (ASDs) and other developmental disabilities. On a personal note, I am involved in some of the most exciting projects of my career, which spans 35 years and a range of activities. My former and current students far surpass me, which is all that a teacher can ask, I have a lovely family, and life is good. I have no axe to grind against the discipline to which I have devoted my professional life. Nonetheless, I have

significant concerns regarding the future of that discipline unless it changes significantly, and soon. The balance of this article describes those concerns and makes some suggestions for increasing the likelihood that the discipline of behavior analysis will survive and prosper. In considering those concerns and suggestions, bear in mind that my prior attempts to predict and improve the future of something of considerable importance to me—my financial status—have yielded resources sufficient to allow me to retire in 676 more years. I’m not a fortune teller, I’m a behavior analyst.

For convenience, I will discuss five general concerns. They are overlapping and somewhat amorphous, but nonetheless serve as convenient points of departure.

*Concern 1.* There aren’t enough of us and we don’t have enough power. ABAI membership has indeed grown, and there are certainly behavior analysts who do not belong to the organization, but the simple truth is that behavior analysis is a small discipline, viewed as outdated and insignificant by mainstream psychology and, if it’s recognized at all, by the world at large. Most laypeople who have heard of behavior analysis, like most psychologists, believe that the field had its heyday in the 1960s

---

Address correspondence to Alan Poling, Department of Psychology, Western Michigan University, Kalamazoo, Michigan 49008 (e-mail: alan.poling@wmich.edu).

and that it died with, if not before, B. F. Skinner.

Overall, we behavior analysts have not done a good job of selling ourselves and our discipline, or converting the masses (or, for that matter, the influential elite) to our point of view. For all of his strengths, Barack Obama is not a behavior analyst. Neither is my mechanic, my physician, or my children's teachers.

At the present time, I am conducting research with APOPO, a small organization based in Morogoro, Tanzania, that teaches giant African pouched rats to detect land mines and tuberculosis. It is impressive, yet dismaying, to see that APOPO has marketed itself and its Herorats far more effectively than we behavior analysts have marketed ourselves or our field. In fairness, some people—my friend and colleague Dick Malott is the quintessential example—have devoted their lives to recruiting initiates and turning them into competent and committed behavior analysts. I strongly believe that more of us should follow their lead. Our field needs good salespeople just as much as it needs capable scientists and practitioners. Unfortunately, at present there are no jobs for salespeople in our field. Research, teaching, and clinical impact trump recruitment every time. Perhaps those of us on hiring, tenure, and promotion committees need to reexamine our priorities.

*Concern 2.* Applied behavior analysis is becoming synonymous with treatments for ASDs. The value of applied behavior analysis in improving the behavior, hence the quality of life, of people with ASDs is clear and impressive, especially when early and intensive interventions are arranged (Matson & Mayville, in press). This effectiveness has rightly generated a market for competent behavior analysts, and holding the BCBA has come to be rather widely recognized as a marker of competency. Both are good things.

The problem is that applied behavior analysis is useful in treating people with a wide range of diagnoses, as well as in improving the behavior of people to whom no diagnostic label is applied, but most of the current emphasis in applied behavior analysis is on helping people with ASDs. As an example, ABAI sponsors a yearly conference dedicated to autism, and the majority of presentations at its annual general convention address the same topic. If in the near future a drug company or biotechnology firm develops a truly effective intervention for treating ASD, its introduction will sever the jugular of applied behavior analysis.

To ensure that behavior analysis survives, those of us committed to the field must address a broad range of significant problems. Of such, there is no shortage. Obvious, and gargantuan, behavioral problems include overpopulation, global warming, obesity in affluent countries, famine and disease in poor countries, and genocide. Solving these problems will not be easy—the low-hanging fruit was picked long ago—but substantial rewards await the people, and the disciplines, that develop the solutions. It is my hope that behavior analysts, and behavior analysis, are foremost among them. For that to occur, however, our applied training programs must develop students' interest and expertise in more than ASDs. Moreover, those training programs must enable graduates to create, for themselves if necessary, positions in which their skills can be utilized and to work effectively with people of very different orientations. Knowledge of functional analysis and multiple baseline designs is useful, nay invaluable, for any applied behavior analyst, but much more is required to "save the world," as Dick Malott has championed for so many decades, and with it our field.

*Concern 3.* EAB now stands for esoteric behavior analysis. Years ago, I was trained in the experimental

analysis of behavior (EAB) by Andy Lattal, one of the best behavior analysts and best men I have known. He guided me gently through Honig (1966) and Sidman (1960). I entered as a hippie existentialist and emerged as a behavior analyst. The general approach to studying and explaining behavior developed by Skinner and evident in those books is uniquely powerful and its application helped to reveal, clarify, and illustrate the principles of behavior that remain the foundation of behavior analysis. Controlled laboratory research in the Skinnerian tradition (i.e., EAB) played a valuable role in the early development of our field. Most of that research was conducted with rat and pigeon subjects and published in the *Journal of the Experimental Analysis of Behavior (JEAB)*.

But from the earliest days of *JEAB* to the present, many basic research studies are not obviously relevant to significant actions of people and other animals in their natural environments. In fact, many established areas of basic research involve little or nothing more than a progression of well-controlled studies that clearly demonstrate the influence of unimportant independent variables on trifling outcome measures. I know, because I have done more than my share of such work.

A good example involves the study of various anticonvulsant drugs on schedule-controlled responding, a research area that my students and I explored at some length during the 1980s (e.g., Poling & Picker, 1987). In brief, our findings and those of other researchers revealed—big surprise—that different anticonvulsants produced different effects and that the effects of a given drug often depended on the specific schedule that we arranged. Those studies were conducted as part of grant-funded research designed to profile in rats and pigeons potential behavioral side effects of drugs used clinically to treat epilepsy in humans. In the grant

proposals we argued, apparently persuasively, that the use of laboratory animals allowed better experimental control than could be obtained in experiments with humans and that findings with nonhumans would provide meaningful information about adverse effects likely to occur in human patients. Some of our data, for instance, those examining drug effects on reaction time and the acquisition and performance of conditional discriminations (“learning” and “memory”), probably did so. But the data resulting from studies of schedule-controlled responding did not, save in the trivial sense of showing that all of the drugs we evaluated were behaviorally active and, if given at sufficient doses, nonselectively reduced responding. More subtle effects, in which various elements of the schedules in effect modulated quantitative or qualitative drug effects, were interesting from an analytical perspective, but their application to the everyday lives of people with epilepsy were not clear. They still aren’t. Recurrent and consistent free-operant schedules of reinforcement such as those we arranged for rats and pigeons rarely, if ever, occur in the everyday human environment, and it is not at all clear to me how subtle drug effects observed under such schedules relate to the potential harm (or benefit) these drugs might cause a person with epilepsy.

Of course, at the time we proposed the studies, my students and I believed otherwise. For many years, schedule-controlled responding had been widely accepted by behavioral pharmacologists as a sensitive and meaningful index of drug action (e.g., Thompson & Schuster, 1968), and we were trained in that tradition. Only with substantial experience and many failed attempts to find schedules outside the laboratory (e.g., Poling & Foster, 1993), did I come to question the validity of schedule-controlled responding as a technique

for profiling meaningful drug effects in humans.

Regardless of its merit, our work with anticonvulsants was rewarded with money, publications, tenure and promotion, praise, and good jobs for several students. We began and continued the work because it was supported by a social community and ended it when it no longer paid off (i.e., our grant proposals weren't funded). When I reminisce with former students, one or another of us sometimes semijokingly remarks that the anticonvulsant work was actually applied, in that it directly benefited participants—us. The research efforts of behavior analysts, like most significant human responses, are determined primarily by their past consequences and by descriptions of those and related consequences. Researchers work in areas in which they have had success, which may or may not involve topics that contribute to the growth and survival of their discipline.

Basic behavioral research is relatively inexpensive—the cost of a single nuclear submarine would easily cover all of it that has been done in the past 50 years and will be done in the next 50—and I have no quarrel whatsoever with colleagues who study behavioral (or other) phenomena that interest them, no matter how trifling those phenomena appear to other people. I do, however, invite us all to recall two warnings: One is: A project not worth doing is not worth doing well. The other: Don't slice bologna with a microtome.

I began my career with EAB, I delight in the precision and control that its experimental procedures afford, I take comfort from the orderliness of its data, regardless of their practical significance, and I hope the field remains viable for a long, long time. I also hope, and fervently, that more basic researchers attempt to answer experimental questions that relate to applied issues and describe clearly, at least now and again, why

their work is important enough to merit support.

Travis Thompson, a superb behavior analyst who comes as close to being a true Renaissance man as anyone can, told his students, including me, to “find a disease” if they wanted their research to be meaningful and supported by mainstream society (which would be willing to pay for it). By “find a disease” he meant that the research should relate in a real and obvious way to an issue recognized by taxpayers to be of pressing importance. The bigger the issue, he said, the greater the potential payoff. I failed to follow Travis's advice, but it is sage, and aspiring researchers would do well to heed it. If they do and they are to succeed, those of us who evaluate junior scientists may have to alter our standards, recognizing that it is easier to publish a dozen well-controlled studies that examine trifles than to publish one that moves us closer to solving a large and thorny behavioral problem. If their solutions were easy, such problems wouldn't exist.

*Concern 4.* Too much “applied” research really isn't applied. I've subscribed to the flagship journals of our discipline (i.e., the *Journal of Applied Behavior Analysis*, *JABA*, representing the applied research area, and *JEAB*, representing the basic research area) for longer than I care to remember. I skim the contents of each new volume I receive and usually read something. Occasionally, doing so seems like a form of penance—the atheist's equivalent of 10 Our Fathers and 30 Hail Marys—but often I find an article that makes me feel good about our field. Sometimes I find several. Interestingly, in recent years I've found an ever-increasing number of *JABA* papers that seem better suited to *JEAB*, in that it is not at all clear how they are relevant to the well-being of participants or how they relate to socially significant behaviors. To me, and consistent with how

the term was used in Baer, Wolf, and Risley's seminal article (1968), studies that lack these characteristics are not truly applied. Although basic and applied research are better viewed as a continuum than as a dichotomy, to the extent that the distinction is useful I would argue that *JABA* has become less applied.

From 1972 through 1974, I was a master's student in the psychology program at West Virginia University (the program, like the state, was and is fantastic—go Mountaineers!). As with many programs then and now, WVU's program was roughly divided into two camps, experimental and clinical. As a student of Andy Lattal's, I was proudly in the former. During long and intellectually stimulating nights at the Blue Tick Tavern, we experimental students laughed behind their backs at our applied peers, who we derisively described as "trying to save the world with a sack of M&Ms and a cattle prod." The cattle prod is long gone, but as I read *JABA* today I sometimes get a déjà vu feeling and remark to myself, "Reinforcement still works and there are lots of colors and flavors of M&Ms." Although I laughed at that message in 1973, I don't today. Reinforcement, and other elementary principles of behavior, can be used to help a huge range of people in myriad ways, and that is no laughing matter. Demonstrating this may not be big science, but it is a tremendous humanitarian accomplishment. I've already argued that behavior analysts should "get a disease" (in addition to ASDs) in the sense that Travis Thompson recommended, and the best *JABA* articles reflect the work of people who obviously have done so. A case in point is the work of Steve Hayes, who has used relational frame theory and acceptance and commitment theory, which he developed, to explain and treat a wide range of clinical disorders. Steve, by the way, was in the clinical camp at

West Virginia University when I was a student there.

Although I have not attempted a count, it appears to me that in the past decade an increasing proportion of *JABA* articles are not focused on directly helping people, but rather on determining whether the same relations between environmental inputs and behavioral outputs demonstrated in *JEAB* studies hold in people with special needs. In principle, there is nothing wrong with this strategy. In fact, given that basic research should provide a foundation for applications, it is commendable. Translational (or bridge) research, which involves using basic research findings to develop potential interventions that are evaluated in a series of increasingly naturalistic settings, with the goal of ultimately developing treatments with practical utility in the everyday world, has much to recommend it and I have long argued for, not against, this strategy (e.g., Poling, Picker, Grossett, Hall-Johnson, & Holbrook, 1981).

Bridges are built in sections. Metaphorically, in translational research the bridge stretches from the basic research side to the clinical application side, perhaps spanning a chasm filled with ignorance and disorder, and the sections are laid from the former to the latter. It may be that much of what has appeared in *JABA* to date are descriptions of early bridge sections arising from, and firmly anchored to, basic research. That is, they are studies that replicate or modestly extend *JEAB* findings in populations of potential clinical significance. Results of these attempted replications will form the basis for subsequent, and increasingly applied, research. Eventually, the clinical side will be reached and interventions based on basic research findings will be widely used to produce socially valid changes in significant behaviors observed in participants' natural environments. Some researchers (e.g.,



those who attempt to use the matching equation as a tool for developing interventions) appear to be moving in this direction. Their work is to be applauded, and I wish them well.

I fear, however, that researchers in this and other areas can easily get trapped into looking at, and publishing papers describing, small procedural variations that make the metaphorical bridge wider, but do not move it forward. A case in point on which I recently commented critically (Poling, 2010) involves the use of progressive-ratio (PR) schedules to evaluate the potency, strength, or effectiveness of scheduled reinforcers. Under PR schedules, the number of responses required to produce a putative reinforcer increases as a function of the number of reinforcers earned in a session, according to a specified algorithm. For example, the number of correct math problems that must be completed for a child to gain access to a favored toy (i.e., the ratio requirement) might begin at two and double each time a reinforcer (toy access) is earned, so that the number of completed problems required to get the toy would be, in succession, 2, 4, 8, 16, 32, 64, and so on. The session ends when a specified period of time elapses without a response, and the largest ratio completed before this period begins defines the “breaking (or break) point,” which is assumed to measure reinforcer potency.

Reinforcer potency is a hypothetical construct, and prominent behavior analysts have consistently and compellingly argued that hypothetical constructs play no useful role in a science of behavior (e.g., Michael, 2004; Skinner, 1938). Unlike, for example, reinforcer delay or magnitude, reinforcer potency is not measured directly, but is instead inferred on the basis of how the scheduled event interacts with ongoing behavior. PR breaking strength is one measure of this interaction, but many others are reasonable and commonly

used in behavior analysis (e.g., choice, rate of responding). These measures are not necessarily equivalent, and the so-called potency of a given reinforcer often depends on the general procedure used to measure it.

Moreover, the specifics of a given procedure can also influence reinforcer potency. In introducing a recent series of *JABA* articles, Roane (2008) indicates that this appears to be the case with PR schedules, but there are many uninvestigated areas for future research, and he suggests that “almost any previous study that has examined variables that alter the effectiveness of positive reinforcement could be replicated [in applied settings] using PR schedules” (p. 159).

I argued that such research would be of little, if any, practical value, because from an applied perspective the potency of a scheduled reinforcer is primarily important with respect to whether or not that reinforcer can be arranged to improve a socially significant target behavior. How it affects behavior in other circumstances, even in the population of concern, is of value only if this information is easily obtained and leads directly to better interventions. No such benefits have been demonstrated, and no compelling arguments for their existence have been provided. Therefore, given the issues of opportunity cost and the aversiveness of exposure to long response ratios, detailed study of the effects of PR schedules in protected populations does not seem to be warranted. Studies demonstrating that PR schedules are in some sense especially useful for isolating effective reinforcers that can be put to clinical use are, however, well worthwhile from an applied perspective. I hope that *JABA* researchers who are interested in PR schedules aim to produce such studies and remember always that good applied research helps people in addition to those who conduct it. Simply studying participants with a diagnostic label and

special needs does not make a study applied.

Of course, a study does not have to be applied to be significant. Science is a social enterprise, and a community of scientists deems work valuable to the extent that such work is rewarded. *JABA* editors and reviewers obviously find research that I do not construe as applied as sufficiently valuable to be published, and that is fine by me. But we have only a small number of men and women who are capable of doing first-rate behavior-analytical research, and their resources are limited. Put simply, our collective capacity to conduct studies is not large. Earlier in this article, I suggested that esoteric basic research will not help our discipline to survive and grow, but that high-quality basic and applied research that is clearly related to large social issues may do so. It would be unfortunate for our field, but perfectly understandable, if the best and brightest forsook the latter for the former.

*Concern 5.* Our technical language isn't precise and can be harmful. Decades ago, while learning the fundamentals of behavior analysis at West Virginia University, I mastered—at least to the extent that instructors and fellow students stopped bitching at me—the technical vocabulary of the field. I memorized and repeated the alleged virtues of precise, objective scientific terminology and the great dangers of subjective and mentalistic everyday language at every opportunity, including many I contrived. Then I went to Minnesota, MA in hand, to pursue the PhD.

And I learned soon enough that folks talk differently in Minnesota and West Virginia, in more ways than using “eh” to end every sentence and not using “holler” to refer to a small valley. Lo and behold, and contrary to what I'd learned to hold sacred, some of the highly able women and men who were my teachers and fellow students used terms like *ex-*

*pectancies*, *fixed action patterns*, and *species-typical behaviors*. Even worse, they used what I viewed as fundamental behavior-analytic words in horrible ways, like calling all unconditional stimuli *reinforcers*. They didn't faithfully restrict *evoke* to operant relations and *elicit* to respondent ones, like I'd been taught to do. But their word use did reflect fairly precise stimulus control, and many of their terms were only shorthand descriptions of real and general relations between specified classes of environmental (and sometimes physiological or genetic) inputs and behavioral outputs. Moreover, their verbiage appeared to control behaviors that were at least as gainful at those controlled by the Skinnerian language I had learned.

At least, the people at Minnesota were generating orderly data and answering interesting experimental questions. For example, three faculty members, Gail Peterson, Bruce Overmier, and Milt Trapold, and their students formed the heart of the association learning group and did a lot of work with conditional discriminations. To the surprise of the hard-nosed behavior analysts, including me, they were finding faster acquisition and greater terminal accuracy when consequences were differential, in the sense of being determined on the basis of the discriminative stimulus that controlled a correct response (e.g., food following a correct response to red, water following a correct response to white) than when they were nondifferential (e.g., food following 50% of correct responses to both red and white and water following the other 50%). This pattern of results, which came to be called the differential outcomes effect, occurs consistently under a wide range of conditions and is one of the most robust phenomena in discrimination training (Goeters, Blakely, & Poling, 1992). Why it occurs is open to debate. Researchers at the University of Minnesota initially attributed it to

the formation of *expectations* that guide and improve performance when differential outcomes are arranged, and most association learning theorists appear to favor this explanation today. Whether this explanation is adequate is debatable. I, for one, find it unappealing unless there are clear empirical indications of the expectancy (e.g., faster responding in the presence of one of the discriminative stimuli). Be that as it may, my time at Minnesota taught me that the words one uses to describe relations between environmental outputs and behavioral inputs matter much less than the actual relations and that behavioral phenomena do not lose their significance when they are explained in what appear to be inadequate ways.

I left the Psychology Department at Minnesota and, after a brief stop in South Carolina, came to Western Michigan University, where the Psychology Department was then and is now well and truly devoted to behavior analysis. No more association learners, no more ethologists, no more bullshit. Only behavior analysis, pure and strong. Problem was, it came in two versions: Jack Michael's and Dick Malott's. Both were absolutely committed to the field, both were excellent teachers and scholars, and I'm proud to say, both were to become my friends and colleagues. They viewed behavior in similar, but not the same, ways, and they used somewhat different language to describe it. For example, Jack's establishing operations came in more versions and had more specific actions than Dick's. Their students learned what their advisers taught them, and it wasn't hard to tell a Malottian from a Michalike (pronounced "Mike-a-like") in the courses that I taught. Sadly, none of them defined terms in exactly the proper way. For that, they had to be my students, a small but mighty tribe, the Polingians. Of course, the three tribes could converse, but to do so they had

to resort to a kind of pidgin speech not entirely pleasing to anyone. As Jack used to note on students' imperfect responses, it was NQR (not quite right). Our language was precise within, but not between, subcultures, and it was readily apparent that the alleged precision of behavior-analytic nomenclature was an illusion.

Four examples readily illustrate this imprecision: First, some behavior analysts restrict the use of *reinforcement* and *reinforce* to situations in which consequences immediately follow the responses in question. Others, however, do not impose a requirement of immediacy, and a few call situations that involve delayed events "analogues to reinforcement" (see Bradley & Poling, in press; Schlinger, Blakely, Fillhard, & Poling, 1992). Second, *noncontingent reinforcement* is widely used to describe a procedure that involves time-based presentations of stimuli that weaken rather than strengthen the designated response. There is no evidence of reinforcement, and there may or may not be a contingency (Poling & Normand, 1999). Third, *contingency* is used in several confusing and contradictory ways, so that the term is almost meaningless (Lattal & Poling, 1981). Fourth, some behavior analysts use procedural definitions of terms like *reinforcement* and *punishment*, and for them a particular environmental arrangement (e.g., one in which a parent yells each time a child is noncompliant) is sufficient for assigning a given label (e.g., the child's noncompliant behavior is being punished). Other behavior analysts favor process definitions, in which a given environmental arrangement must produce a specified behavioral effect (e.g., a parent yells each time a child is noncompliant and the probability of yelling therefore decreases) before a given label (i.e., punishment) is assigned. Procedural definitions are more liberal and, in my view, less useful (see



Lattal & Poling, 1981). Many other examples of imprecise and potentially confusing verbal behavior within our field are available. Fortunately, despite the imprecision, most members of the community of behavior analysts manage to communicate reasonably well with one another.

They, or at least I, fare less well when talking to people with no training in our field. Consider the phrase *contingencies of reinforcement*, perhaps the most sacred of all of our phrases, the basis for all operant behavior, a term so auspicious as to have graced the cover of a book by B. F. Skinner. Ask a budding, well-trained behavior analysis student what makes people behave as they do and the odds are good that he or she will reply “contingencies of reinforcement.” And that is a great answer. A great answer, that is, if the person asking the question is another behavior analyst. But what if he or she is just an ordinary citizen, perhaps a person with a high school diploma, an undergraduate degree in accounting, or a PhD in chemistry?

It is highly likely that the phrase will be utterly meaningless. *Contingency* isn’t a word most people use or can define. But even if they can define the word, they’ll be lost. According to *Merriam-Webster’s Collegiate Dictionary* (2009), “Contingency: 1: the quality or state of being contingent; 2: a contingent event or condition: as *a* an event (as an emergency) that may but is not certain to occur, *b* something liable to happen as an adjunct to or result of something else.” According to the same source, *contingent* is defined as “1: likely but not certain to happen; 2: not logically necessary; 3a: happening by chance or unforeseen causes, *b*: subject to chance or unseen effects; 4: dependent on or conditioned by something else; 5: not necessitated; determined by free choice.”

What in hell is a contingency, of reinforcement or otherwise? In my view, the term is a bad one, and we’d

be well served as a discipline to stop using *contingent*, *contingency*, and *contingency of reinforcement*. Like the reflex reserve, they’re old and tired and do more harm than good. Our field would be well served to be rid of them, and I have actively avoided their use for 15 years (save for special occasions like running, or these days walking, with Dick Malott). My ability to communicate with students and colleagues has not suffered, and interactions with laypeople have only improved. Making the language we use to describe behavioral relations as simple and straightforward as possible is one way of increasing our appeal, and I highly recommend it. Sacrilege, I know, but worth considering in view of the potential value of communicating clearly with those outside the field whose support is essential for our survival. Please note that I am not arguing for imprecise language or for the total abolition of a specialized language of behavior. I am arguing that such a language should be as simple and straightforward as possible and that people should not get too caught up in the words that they and others use. The meaning of words resides in the responses that they control in other people, and whenever possible we behavior analysts should use language, technical and otherwise, that increases the likelihood that laypeople will understand and support us and our discipline.

### Concluding Comment

At last year’s ABAI conference I presented some of the material on which this article is based. As best I could tell, the audience reaction was generally positive (I probably flatter myself unduly), but there certainly were exceptions. One person became quite emotional when I suggested that *contingencies of reinforcement* is a poor term, one supported by precedent but little else. Another

argued adamantly that bridge research in all of its guises is invaluable and constitutes the future of our discipline. I was pleased that my remarks caused them to reflect on the value of particular practices and that my colleagues were passionate about those topics. Whether they agreed with me was of no concern because my opinions were not closely tied to trustworthy data, but were instead vague musings reflecting little more than the experiences of my life. The same is true of the contents of the present article. They are suggestions for discussion, not directives.

As best I can ascertain, the future success of our discipline depends jointly on the characteristics of the discipline and those of the world in which it exists. Change is unceasing; those organisms that adapt to it survive and, if they are human, carry forward elements of their culture. If they do not, they perish. Consider, for example, technology, which over the past 20 years has strongly influenced how humans interact with each other and with the world. Some of my colleagues, with Ron van Houten notable among them, have embraced the emerging technology and put it to good use in developing applied interventions (in Ron's case, strategies for increasing traffic safety). He is highly successful, as are his students, who will carry the field forward after he is gone, well trained to adapt to and benefit from further technological developments.

In the face of a world likely to change in ways that are impossible to predict, diversity may be the ultimate key to survival. Behavior analysts can do many things well and, as I have suggested previously, it is probably to our current and future advantage as a discipline to broaden our scope as much as possible, especially by focusing on current problems that are likely to endure. The poorly behaved, like the poor, are with us always. To the extent that we can sort out why, and devise strategies for improve-

ment, our future is secure. Barring another asteroid....

## REFERENCES

- Baer, D. M., Wolf, M. M., & Risley, T. R. (1968). Some current dimensions of applied behavior analysis. *Journal of Applied Behavior Analysis*, 1, 90-97.
- Bradley, K. P., & Poling, A. (in press). Defining delayed consequences as reinforcers: Some do, some don't, and nothing changes. *The Analysis of Verbal Behavior*.
- Goeters, S., Blakely, E., & Poling, A. (1992). The differential outcomes effect. *The Psychological Record*, 42, 389-411.
- Honig, W. K. (Ed.). (1966). *Handbook of operant behavior*. New York: Appleton-Century-Crofts.
- Lattal, K., & Poling, A. (1981). Describing response-event relations: Babel revisited. *The Behavior Analyst*, 4, 143-152.
- Matson, J. A., & Mayville, E. (Eds.). (in press). *Behavioral foundations of effective autism treatment*. New York: Sloan.
- Merriam-Webster's Collegiate Dictionary*. (2009). Retrieved from <http://www.merriam-webster.com/>
- Michael, J. L. (2004). *Concepts and principles of behavior analysis* (rev. ed.). Kalamazoo, MI: Association for Behavior Analysis International.
- Poling, A. (2010). Progressive-ratio schedules and applied behavior analysis. *Journal of Applied Behavior Analysis*, 43, 347-349.
- Poling, A., & Foster, T. M. (1993). The matching law and organizational behavior management. *Journal of Organizational Behavior Management*, 14, 83-97.
- Poling, A., & Normand, M. (1999). Noncontingent reinforcement: An inappropriate description of time-based schedules that reduce behavior. *Journal of Applied Behavior Analysis*, 32, 237-238.
- Poling, A., & Picker, M. (1987). Behavioral effects of anticonvulsant drugs. In T. Thompson, P. B. Dews, & J. Barrett (Eds.), *Neurobehavioral pharmacology* (pp. 157-192). Hillsdale, NJ: Erlbaum.
- Poling, A., Picker, M., Grossett, D., Hall-Johnson, E., & Holbrook, M. (1981). The schism between experimental and applied behavior analysis: Is it real and who cares? *The Behavior Analyst*, 4, 93-102.
- Poling, A., Schlinger, H., Starin, S., & Blakely, E. (1990). *Psychology: A behavioral overview*. New York: Plenum.
- Roane, H. (2008). On the applied use of progressive-ratio schedules of reinforcement. *Journal of Applied Behavior Analysis*, 41, 155-161.
- Schlinger, H., Blakely, E., Fillhard, J., & Poling, A. (1992). Defining terms in behavior analysis: Reinforcer and discriminative stimulus. *The Analysis of Verbal Behavior*, 9, 153-161.

- Schlinger, H., & Poling, A. (1998). *Introduction to scientific psychology*. New York: Plenum.
- Sidman, M. (1960). *Tactics of scientific research*. New York: Basic Books.
- Skinner, B. F. (1938). *The behavior of organisms*. New York: Appleton-Century-Crofts.
- Thompson, T., & Schuster, C. R. (1968). *Behavioral pharmacology*. Englewood Cliffs, NJ: Prentice Hall.